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Discussions

Interactive comment on "Primary production sensitivity to phytoplankton light attenuation parameter increases with transient forcing" *by* Karin F. Kvale and Katrin J. Meissner

Anonymous Referee #1

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General comments:

The paper "Primary production sensitivity to phytoplankton light attenuation parameter increases with transient forcing" by Kvale and Meissner addresses a topic that is both relevant and within the scope of BG, namely, the sensitivity of simulated marine biogeochemistry to changes in the parametrization of the under water light field under climate change. In particular, the study addresses the sensitivity of marine net primary production (NPP), alkalinity, dissolved inorganic carbon, phosphate, nitrate, and oxygen, as modelled with the University of Victoria Earth System Climate Model (UVIC ESCM) in pre-industrial equilibrium as well as in historical and future climate change simulations. The main finding of the study seems to be that in the pre-industrial equi-

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librium simulations the sensitivity of the modelled NPP to changes in the phytoplankton light attenuation parameter is rather low, while it increases with climate forcing.

While the paper reads well in most parts and is reasonably well structured, both the scientific question that it aims to address and the implications of the main finding could be made more clear. Open documentation of model testing and tuning is clearly desirable and the authors' recommendation to test parameter sensitivities not only in steady-state, but also transient simulations seems reasonable. Yet, I recommend to address several remarks as outlined below before the paper can be published in BG.

The fact that the modelled primary production is sensitive to a change in the value for the light attenuation parameter is not very surprising. What is the scientific basis for the choice of the parameter k_c? Why not a different parameter? What is the relationship to the Chl : C ratio, which is assumed to be a global constant in this study(?) ?

Maybe the authors could address the question what such a sensitivity test actually tells us. Does it tell us something about the real world? Or rather about the model and its applicability? Is this sensitivity specific to UVic, to EMICs? What can we learn from this sensitivity study about the different regimes of phytoplankton growth limitation (light, nutrients)?

It would be really interesting if one could show that for a range of values for a parameter the observed data can be reproduced reasonably well, but that for the same range the sensitivity is very large for a future climate simulation. However, this does not seem to be the case here, since the results seem to illustrate that for no single parameter value the match to observations is convincing. (Lower k_c values seem to fit better to surface chlorophyll, higher ones better to biogeochemical tracer profiles.)

If the main point of the paper is the increasing sensitivity of modelled primary production with increasing forcing to changes in the value of the parameter k_c, then this should be supported more clearly by the shown figures. It is difficult to see how the authors arrive at the conclusion that the sensitivity to k_c is generally modest for the steady-state. There seems to be quite a spread in some simulated variables.

It would be interesting to see the sensitivity of both the physical and the biogeochemical model part to changes in k_c if the shortwave heating were included.

What do other studies say about the sensitivity of NPP to changes in the light field? What do other studies say about the sensitivity of oxygen to the changes in NPP? This could be further elaborated in the discussion section.

Specific comments:

Introduction: The discussion of underwater light field descriptions seems to be rather long and not taken up in later sections of the paper. Furthermore, Manizza et al (2005) and Gnanadesikan and Anderson (2009) study the effect of including the shortwave heating by phytoplankton and others in their models, and also Kim et al (2015) have the shortwave heating included in their model. This effect is not addressed at all in the rest of the present paper.

Section 2, Methods:

- p3, lines 16-19: As far as I understand, the erroneous calculation of the light attenuation with depth was treated and fixed in Keller et al (2012). It would be helpful to be more specific here and clarify that in the present paper the corrected version of the model as described in Keller et al (2012) is used. In the current manuscript it says that "This calculation is corrected here."

- p3, line 20ff: This sentence is not clear and should be split into at least two parts. The authors should make clear that the sensitivity of biogeochemistry *to* different values of k_c both in a steady-state simulation and in a transient simulation - and not *to* k_c and *to* climate forcing - is assessed. Also, the authors should clarify that the transient simulation is not only a historical one, but also includes a future scenario extending to the year 2300. In addition, the description of the forcing could be more specific (e.g., what are "historical atmospheric CO_2 changes"? Is the model driven by CO_2 concentrations or emissions? What are “agricultural” emissions? CO_2 emissions due to land use? Is the model forced by CFC emissions or concentrations? etc.) Should it be changes *in* land ice instead of *to*?

- p3, line 24: replace “models were” with “simulations were” or with “model was”?

- p3, line 25: Is the non-CO_2 GHG radiative forcing prescribed or calculated in the model from the concentrations? Or from the emissions? What does “forced using . . . fractions of the land surface devoted to agricultural uses” mean? How is the land use forcing realized here?

- p4, line 7: I do not understand this sentence. Replacing the default value of which parameter with which values results in the different shown values for k_c? Do the authors want to say that the 3 given references give different values (or a range) for k_c, or am I missing anything here?

- p.4, line 11: Please clarify what is meant by “any value assigned to k_c is going to be highly model-dependent”.

- p4, line 12/13: What conversion factor for Chl : N was used and why?

- The description of the observational datasets could be moved to the methods section, and could be more specific (which tracer data are taken from which dataset).

Section 3, Results:

- p4, line 19ff: I am not sure how the provided Figure 1 showing surface chlorophyll illustrates the sensitivity of primary production. Why don’t the authors show simulated (vertically integrated) primary production? Also, the authors could explain why they are comparing satellite data to results from simulations at pre-industrial conditions.

- p4, line 22/23: The authors write that chlorophyll is overestimated in the simulations compared to satellite data in the tropics and the southern hemisphere mid latitudes, but in the plot it is the tropics and the *northern* hemisphere mid latitudes (∼35-70

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degrees) that are overestimated. The authors should check the latitude axes of the plotted data.

- p4, line 27ff: Why is the effect of increasing k_c on surface chlorophyll ("biomass" as written in the text is not shown anywhere in the plots) negative in the Southern Ocean, but positive elsewhere? A more detailed explanation of the mechanisms including the vertical distributions and the different regimes of nutrient/light-limitiation in the different regions would be helpful here. Note that the cited study by Kim et al (2015) shows an increase of surface chlorophyll due to the inclusion of light attenuation by colored detrital matter almost everywhere.

- p4, line 32ff: It would be helpful to see the primary production (profiles) to follow the discussion in this paragraph. Also, it should be clarified whether the purpose of this paragraph is to get a better understanding of the primary production sensitivity or of the consequences this sensitivity has on the distributions of the biogeochemical tracers that are shown.

- p5, line 11ff: What do the authors conclude from the fact that K1 fits best for surface chlorophyll (as stated on p.4, line 24), but K4 and higher fit better for the deep ocean biogeochemical tracers when compared to data from SeaWiFS, WOA, and GLODAP?

- p5, line 16: Since the physical response is the same in all simulations, it seems that in the model there is no effect of the underwater light field on temperature, which may be worth mentioning somewhere in the paper.

- p5, lines 24ff: From the given plots it is hard to see a decline in chlorophyll prior to 2100 in the low latitudes. Also, the terms "NPP", "biomass" and chlorophyll seem to be used interchangeably here.

- p5 line 29: Please specify "both of these regions".

- p5, line 30: What about the peak at ∼80S that is decreased from steady-state to 2100?

- p5, line 31: It is hard to see any differences in chlorophyll in K8 between the steady-state shown in Fig1 and the 2100 state in Fig5.

- p6, line 7: Why does chlorophyll decrease from 2100 to 2300 in most simulations north of 40N?

- p6, line 17f: The spreads in the simulations for different times would be easier to compare to each other if the plots used the same scale. Currently Fig1 uses differences to observations and Fig6 uses absolute values. And why is it unsurprising that the spread is increasing in global NPP, but not in the biogeochemical tracers?

- p6, line 20: It is hard to see the increasing spread in the Southern Ocean from the given plots.

- p6, line 23: The sentence "For all biogeochemical quantities, simulated spread at the surface increases with time." seems to contradict the earlier one saying that the spread in the profiles is retained over time.

Section 4, Discussion:

- p7, line 7: I am not sure the results convincingly show that the value of k_c matters little for primary production in the pre-industrial steady-state of the model for values above 0.04 m^2 / mmol N, but matter more for lower k_c values.

- p7, line 13f: Please clarify what is meant by this sentence ("That this is true...").

- p7, line 25ff: Please explain in more detail how this study demonstrates the importance of which mechanism. - This section could benefit from a quick language check. Some words seem to be missing.

Section 5 Conclusions:

- p7, line 29ff: Saying that the sensitivity is substantial also in steady-state seems to contradict to what has been stated above (that it matters little, see p7, line 7).

Do the terms “steady-state”, “equilibrium” and “pre-industrial” all refer to the same simulation? The terms could be used more consistently in the paper.

Figure 1: Why are the chlorophyll profiles (probably global means?) shown in the right panel of Figure 1 not discussed in the manuscript? From this plot it seems that the global mean response is an increase of chlorophyll at the surface and a decrease subsurface for increasing values of k_c. Also, in this plot it seems that there are only 3 model layers shown in the upper 200 m. Is this the vertical resolution of the model? If so, it would be worth mentioning such a coarse resolution in a study that is on the vertical distribution of light in the upper water column. Furthermore, the latitude axis of the plotted data should be checked.

Figure 3: Why is the global alkalinity for K8 so different from the other Ks (especially for the deep ocean)?

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